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that variation has not occurred merely by large losses subsequently fractionated so as to form intermediates. Not merely intermediates arise but also those which transcend any known original sports. Original black races have become blacker; original yellow races have become yellower; white-spotted races have become more spotted still at the will of the breeder. Large races also become larger, and small races smaller, under the hands of the fancier. He does not limit himself to the production of intermediates.

To suggest further that all variation transcending limits previously existing is due to loss of inhibitors and so is really retrogressive is scarcely satisfying. It is a formal evasion of the difficulty but in no sense a solution of it. It belongs with the box-within-box idea of development. I agree with Bateson that variety formation within the higher animals and plants seems to be very frequently by a process of loss but I can not believe that this is the exclusive process concerned in the formation of new species or even of varieties. It needs but to carry the idea to its logical conclusion to show its absurdity. Is man merely an amoeba simplified by loss of inhibitors? I can not believe so. I can not believe that the original proteid molecule has since its original synthesis only grown simpler. New radicles have undoubtedly become attached to it as side-chains replacing or supplementing old ones and changing its properties. The living substance is not merely *losing* constituents; it is also gaining new ones. Similarly organisms, morphologically and physiologically, change not merely by losses but also by gains. It is impossible to explain evolution satisfactorily by either process alone. The two go hand-in-hand and no doubt are constantly occurring among organisms. Change is universal. Mere subdivision of a species into two groups of individuals, which are prevented from intermingling, seems to be sufficient in time to make the two groups specifically distinct. Each keeps on changing in so many different ways that it would be little short of a miracle if both changed similarly and simultaneously in all respects. Direct en-

vironmental effects are insufficient to account for such organic changes, for among the best-known illustrations of divergent evolution are the animals of oceanic islands, close together and subjected to the same climatic agencies, undoubtedly descended from common ancestors at no remote period, yet having become distinct, probably through numerous spontaneous changes which isolation prevented from being ground down to a common level by inter-crossing.

These are commonplaces of evolutionary knowledge, familiar to everyone since Darwin and Wallace first called attention to them, yet we are in danger of overlooking them for the moment in our enthusiasm over a new method of attacking the obstinate problems of evolution. It may not be superfluous therefore to call renewed attention to them in this connection. Spontaneous variation is still with us and is as widespread as it was in Darwin's time. It is doubtful whether unvarying "completely homozygous" organisms occur anywhere outside the text-books. In the case of organisms known to be varying genetically there is abundant evidence that small variations are heritable no less than large ones, and we are by no means "done with the idea" that small variations are capable of summation.

With Bateson we must deplore the necessity of engaging merely in destructive criticism. It is indeed "a low kind of work." It would be so much easier, pleasanter, and more satisfying to adopt a single explanatory principle for evolution and build on this. But it would be foolish to go on building lofty superstructures of hypothesis on an insecure foundation, and the more carefully we scrutinize the mutation theory the more serious do our doubts become whether it is a secure foundation for evolutionists to build on. W. E. CASTLE

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#### MASTODON TUSK IN GLACIAL GRAVELS

TO THE EDITOR OF SCIENCE: A tusk of a proboscidean, probably *Mastodon americanus*, was

found recently in a gravel pit in Pony Hollow, twelve miles southwest of Ithaca, N. Y., on the property of Mr. Bert Drake. Unlike most *Mastodon* finds from this region this is not postglacial. It was found in place twenty-four feet below the surface in stratified sand and gravel which was being used in good roads work. The pit is in the base of an extensive terrace whose top follows the valley wall high above the outwash gravel plain which occupies the floor of the valley. The exact origin of this Pleistocene terrace is obscure but it is certainly not later than the end of the ice occupation of the valley and may be earlier.

The tusk was broken in removing the gravel. Two pieces, each about a foot long, from ten to thirteen inches in circumference, were presented to the Paleontological Museum of Cornell by E. A. Dahmen, the road engineer. Three approximate measurements of the curvature of the tusk gave from two feet one inch to two feet eleven inches as the radius of curvature.

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#### SCIENTIFIC BOOKS

##### THE TRANSLOCATION OF MATERIAL IN DYING LEAVES<sup>1</sup>

THE fact of an autumnal transfer of nutrient matter from leaves was first clearly stated by Sachs, in 1863. Sachs's statement was based on microscopical examinations of the leaves of a series of plants in various stages of their autumnal changes, whereby he determined that starch and chlorophyll disappeared from leaves before their fall. He extended this observation to cover most of the other nutrient materials in the leaf. Swart, however, holds that the solution of this question in its broader sense is to be had only by chemical analyses such as he has made.

According to Swart, the first essential to a correct answer is a correct wording of the problem, as follows:

<sup>1</sup> Swart, Dr. Nicolas, "Die Stoffwanderung in ablebenden Blättern," pp. 1-118, Taf. 5, Jena, Verlag von Gustav Fischer, 1914.

During the autumnal coloring of leaves can there be determined, by chemical analysis, a translocation of nutrient materials during the period extending from shortly before to directly after the close of the yellowing?

It is essential to draw the time limits thus sharply, since this period represents a sharply limited, externally recognizable terminal period in the life of the leaf, which in addition to anatomical variations in the petiole, may be directly recognized by the physiological processes which are indicated to us by the disappearance of the chlorophyll. If we would answer the question as to whether, before the fall of the leaf, there may be demonstrated a transfer of any substances, it is necessary to regard only the period during which the leaf, as indicated by these externally recognizable processes, prepares for its final act. After answering this narrower question, then we should consider the amount of these materials in the leaf at other periods of the year, in order to arrive at a causal explanation of the phenomena.

The necessary chemical analyses fall into two groups: those which extend over the entire vegetative period of the leaves, and those which cover only the period directly before and after yellowing. In most of the former investigations the leaves analyzed began to be removed from the tree at a late period, but quite independent of the exact time when the coloring began and ended. The value of the results of such analyses must, therefore, be estimated with caution since, even in most favorable cases, they can only give us an answer to the question of whether, in general, yellow leaves are poorer in their content of any given substance than green leaves of any earlier period. Thus a maximum or a minimum in the proportion of the given material in the intervening period would give results entirely misleading with reference to the question whose answer is sought.

Another objection to former researches that extend over the entire vegetative period is that very frequently a large number of leaves for study were taken at one time from the same